Minutes for the
High-Energy Physics Advisory Panel Meeting
September 23-24, 2004
Hilton Washington Embassy Row, Washington, D.C.

HEPAP members present:

Keith Baker
Joel Butler
Alex Dragt
Frederick Gilman, Chair
JoAnne Hewett
Young-Kee Kim
Paul Langacker
Peter Meyers
Stephen Peggs
Steven Ritz
Randal Ruchti

Also participating:

Barry Barish, Director, LIGO Laboratory, California Institute of Technology
Frederick Bernthal, President, Universities Research Association
Raymond (Chip) Brock, Department of Physics and Astronomy, Michigan State University
Aesook Byon-Wagner, Senior Program Officer, HEP, USDOE
Sekhar Chivukula, Department of Physics and Astronomy, Michigan State University
Glen Crawford, Senior Program Officer, HEP, USDOE
Sally Dawson, Physics Department, Brookhaven National Laboratory
Joseph Dehmer, Division Director, MPS/PHY, National Science Foundation
Jonathan Dorfan, Director, Stanford Linear Accelerator Center
Lyn Evans, Project Leader, Large Hadron Collider, CERN
Thomas Ferbel, HEP, USDOE
Garth Illingworth, Astronomer, University of California Observatories/Lick Observatory
Boris Kayser, Cochairman, American Physical Society Neutrino Study
Thomas Kirk, Associate Laboratory Director, Brookhaven National Laboratory
Joseph Kroll, Department of Physics and Astronomy, University of Pennsylvania
Daniel Lehman, Director, Construction Management Support Division, SC, USDOE
Usha Mallik, Department of Physics and Astronomy, University of Iowa
Marsha Marsden, HEP, USDOE
Hugh Montgomery, Associate Director for Research, Fermi National Accelerator Laboratory
Piermaria Oddone, Deputy Director, Lawrence Berkeley National Laboratory
F. M. O'Hara, Jr., HEPAP Recording Secretary
Rene Ong, Department of Physics and Astronomy, University of California at Los Angeles
Joel Parriott, Program Examiner, Office of Management and Budget
Lawrence Price, HEP Division Director, Argonne National Laboratory
Peter Rosen, Senior Manager for Research Policy, Office of Science, USDOE
Mary Anne Scott, Computer Scientist, ASCR, USDOE
Abraham Seiden, Director, Physics Department, University of California at Santa Cruz
Elizabeth Simmons, Director, Lyman Briggs School of Science, Michigan State University
Robin Staffin, Associate Director for High-Energy Physics, USDOE
Sheldon Stone, Physics Department, Syracuse University
Bruce Strauss, HEP, USDOE, HEPAP Executive Secretary
Maury Tigner, Physics Department, Cornell University
Michael Turner, Assistant Director, MPS/OAD, National Science Foundation
James Whitmore, program Director, MPS/PHY, National Science Foundation
Michael Witherell, Director, Fermi National Accelerator Laboratory
Andreen Witt, Oak Ridge Institute for Science and Education

About 80 others were in attendance during the course of the two-day meeting.

HEPAP Chairman **Frederick Gilman** called the meeting to order at 9:10 a.m. and declared that a quorum was present. He reviewed the agenda and introduced **Robin Staffin** to present the perspective of DOE.

The staff is heavily engaged in the FY06 budget process. The FY05 budget is in the President's request on Capitol Hill. The FY06 budget is in a high state of flux. The *Quantum Universe* report, which describes the quest to explain the universe in terms of quantum physics, has come out and is a success in communicating the excitement of this field and the revolutions to come. The National Academy of Sciences (NAS) high-energy physics study has been started. It has a broad and distinguished membership and could be a very influential study. A number of DOE Offices use accelerators, so a joint panel is focusing on superconducting radiofrequency- (rf-) based accelerators.

Gilman introduced **Michael Turner** to present the perspectives of the National Science Foundation (NSF). Arden Bement has been nominated to be the Director of NSF. He has a long experience both in academia and in the organization and has been an engaged acting director. The EPP2010 study was initiated to identify, articulate, and prioritize the opportunities and activities of the field and to develop a long-term plan. That committee is populated by very influential people and will meet for the first time on November 30-December 1. The Linear Collider technology choice is progressing. Funding of operations, maintenance, and computing for the Large Hadron Collider (LHC) is a critical investment in high-energy physics. The LHC has a stunning science potential. This opportunity came with a bargain price tag. The total U.S. contribution is $531 million, which is divided between DOE ($450 million) and NSF ($81 million). Operations, maintenance, and computing are essential for getting the science out. NSF understands that the discoveries are likely to come very early and that those items have to be funded and cannot be put off. Funding had hit several bumps in the road: A number of plans had been put forward, and new personnel were on board at NSF and DOE. The operations numbers were originally underestimated and have now been increased. The confusion has been resolved. The new budget numbers were received, and DOE and NSF are now committed to a funding profile. The budget figures for FY07 and beyond are not yet decided on, but profiles for FY04 to FY06 that will allow the collaboration to do what it wants to do have been committed to. Arriving at this plan involved cooperation among NSF, DOE, and the US Atlas and Compact Muon Solenoid (CMS) collaborations to make sure the full potential of the LHC is realized.

A big increase has occurred in the NSF budget since 1998, but from 2006 on, the projected budget drops. The House and Senate have marked up the President's request for FY05. The House marked it down 5%. The Senate approved the full budget but did not add any new projects [i.e., Rare Symmetry-Violating Processes (RSVP)]. A continuing resolution is likely until after the election, and then the House and Senate will have to work out their differences. The deficit makes up 20% of the full budget.

NSF has a strategic plan, and several of its goals map directly onto high-energy physics.

A new task for HEPAP is to address the complementarity of the ILC (the International Linear Collider) and the LHC.

Langacker noted that the timing is fortunate; a current large report has been completed on the topic of the new task and can form the basis of the report requested. Turner emphasized that the report requested from HEPAP is to be designed for nonspecialists and the large report is very much by and for scientists.

Ritz asked about the difference between the "bare-bones" and "leadership" budgets and how the choice will be made. Turner replied that the bare-bones budget is what is needed to operate the facilities. The leadership budget has funding for data distribution and analysis centers. NSF funding is committed and is included in the President's request. The NSF is also looking for additional funds for computing resources through the Directorate for Computer and Information Science and Engineering (CISE).

Kirk asked if the *Physics of the Universe* plan was still in effect. Turner responded that it is still in play and that NSF is making good progress to fund the projects described in that report. Task forces are being formed on the various topics. It is a document from the Executive Office of the President; it will not be ignored.

Kim asked if more time could be devoted to the Linear Collider. Gilman responded that some discussion would occur later in the day; at the next meeting, a lot of time will be devoted to the ILC. Staffin noted that several presentations slated later in the day would bear on that topic.

Staffin stated that DOE has a long-term plan for high-energy physics. It contains goals for each fiscal year. The House mark for HEP is $16 million more than the President's request, although the final appropriations bill will almost certainly have adjustments to that.

Gilman declared a break at 9:56 a.m. He called the meeting back into session at 10:13 a.m. and introduced Lyn **Evans** on a video link from CERN.

Evans showed pictures of the LHC project and reviewed the progress. Delivery of components for Cable 1 is progressing well; they are two-thirds of the way through cable production for both Cable 1 and Cable 2. Dipoles were considered to be very difficult to acquire; that turned out not to be the case. A slight drop-off in dipole production occurred during summer vacations; otherwise, progress is advancing steadily. One-third of the whole production of cryogenic dipoles is already delivered to CERN, and those dipoles have been cold-tested at a rate parallel to the delivery rate. Results are excellent. Quadrupole manufacturing has been on schedule; a bottleneck occurred in getting them integrated. That problem has been solved, and integration will soon catch up to delivery. Field testing is very time-consuming. The process will soon move into a sampling mode for cold testing the quadrupoles; all magnets will be tested warm. In the surface and shaft cryogenic systems, most elements are installed and commissioned. In the cavern and tunnel cryogenic systems, most have been delivered, some are being installed, and everything is at least under fabrication.

The Cryogenic Ring Line (QRL) has a bad history. Installation started on July 21, 2003. It was suspended from October 9 to November 24, 2003, because of conflicts between AirLiquide (AL) and its subcontractor. Construction was restarted on November 24, 2003. The contract between AL and the subcontractor was dissolved on January 21, 2004, and a new contract was signed with Agintis on January 26, 2004. Agintis was declared insolvent on March 8, 2004. Installation was suspended on May 3, 2004, because of many quality problems, including geometry, weld quality, and procedures.

Early in June 2004, a leak was detected on a pipe element. Extraction of the pipe bundle in mid-June revealed damaged tables that supported the pipe bundle. Two tables were cracked in the pipe element that was disassembled. At the end of June 2004, an endoscopic examination of other pipe elements revealed damaged tables in many pipe elements and service modules. Half of the tables were cracked. A CERN investigation revealed that the tables had been molded with a different epoxy material than what was specified. The shock resistance of the material employed was an order of magnitude too low, and the components were susceptible to shock fracture. In July 2004, a CERN task force started to verify the QRL design. A production-restart review was initiated on September 15, 2004. Installation will restart November 1.

About 600 pipe modules and 70 service modules will need to be repaired. If it is done by AL, their production facility will be completely saturated. Therefore, the repair will be made at CERN in the main workshop for service modules; correcting the pipe modules will be conducted under the dipole-cryostating contract. A large amount of manpower will be available during a 4-week shutdown of SM18. CERN is preparing for a concentrated repair effort, including working during the Christmas closure. About half of the tables will be able to be repaired; the others will be repaired at a more leisurely rate. The consequence on the schedule can only be clearly assessed when installation restarts and is running smoothly.

Material was conformance-qualified for component manufacture, and production was restarted September 15. The task force has verified the mechanical design of the line to ensure that there are no more weak points. A facility will be set up at CERN for the repair of pipe elements (under the ICS cryostating contract) and special elements (in the main workshop). Under good conditions and with close CERN supervision, installation of Sector 8-1 will be restarted November 1. Then there will be a review before restart. Several hundred dipoles are in storage. The staff is determined to complete the ring by the end of 2006. By the end of the year, one will be able to see if that plan is realistic. If not, another contractor will be brought in, and several segments will be worked on in parallel. Local cabling will be installed in Sector 7-8 before the QRL restart.

In summary, component delivery is proceeding at a rate compatible with a startup of the machine in summer 2007. The new QRL problems will cause delays in installation. The impact of these delays can only be reliably evaluated once QRL installation is proceeding smoothly. The biggest worry is the enormous job of commissioning. Safety systems are difficult to commission. CERN would appreciate help if experienced people could be found. They are not losing time because the order of installation has been reversed. Local cabling is now being done before QRL installation to try to minimize the impact of this additional setback. No one doubts that the ring will be completed in 2006 and that startup will occur in 2007.

Kim asked what specific help was needed. Evans responded that CERN needed people experienced in cryogenics, power supplies, and quench protection. It might have to hire and train people on short-term contracts.

Ruchti asked how many people were needed. Evans responded, about 100 were needed, but not that many will be received. The personnel received would be used as a core team that would train others.

Ferbel asked if this situation presented any financial problems. Evans said that the schedule was everything now; if more needed to be spent, it would be spent.

Gilman noted that the video-over-Internet link with CERN was a first for HEPAP and that it worked very well. He introduced the topic of the Office of High-Energy Physics (HEP) operations reviews of major facilities. **Aesook Byon-Wagner** commented that the goals of such reviews were to

1. Obtain a clear understanding of what is required to ensure the maximum scientific output,
2. Give input to out-year planning for the overall HEP program in the era of tight resources, and
3. Get validations for efficient operations or recommendations for possible improvement.

Plans for the future include (1) annual operations reviews for major facilities that would evaluate and validate operations and upgrade plans for accelerators and detectors and (2) annual program reviews for research that would place more emphasis on research and would stress the program aspect of the laboratory.

Daniel Lehman was then introduced to present a summary of the operations reviews of the Fermilab Tevatron and Stanford Linear Accelerator Center (SLAC) B-Factory. The Operations-Review Charge from Staffin called for the review committee to examine all the Fermilab National Accelerator Laboratory (FNAL) Tevatron and SLAC B-Factory activities associated with facility operations supported by the HEP program and to address the following questions:

1. Is laboratory management effectively setting priorities?
2. Are resources sufficient and appropriately allocated?
3. Are there any risks?
4. Is there an ongoing program of self-assessment?
5. Is ES&H (environment, safety, and health) receiving appropriate attention?

This is a difficult charge.

HEP developed this review charge and the unique data needs in consultation with the laboratories. The review subcommittee leaders were encouraged to work with designated laboratory counterparts well in advance of the actual review. Efforts were made to maintain the same review committee for both reviews. The review committees spent two days at each laboratory. The review subcommittees conducted an unusually large number of interviews with a significant cross-section of laboratory personnel.

The Fermilab Tevatron Operations Review Committee was conducted March 16-18, 2004, and the SLAC B-Factory Operations Review Committee was conducted June 15-17, 2004. The findings of the committees are that:

Charge Item 1

- Both laboratories set priorities effectively and are tracking progress.
- Recent successes with their highest-priority projects (Tevatron Run II at Fermilab; B-Factory at SLAC) reflect capabilities to track progress, resolve problems, and communicate with key stakeholders.

Charge Item 2

- Highly dedicated staffs at both laboratories have made heroic efforts leading to success in high-priority projects.
- Fermilab's ability to support proposed upcoming major project transitions is a concern.
- Sustaining the staff's current heroic level of effort at SLAC for the long term is a concern.
- Future workforce issues at both laboratories include concerns with skill mix and an aging demographic.

Charge Item 3

- Both laboratories have significant technical and programmatic challenges regarding performance of upgrades, critical engineering skills, computing challenges, etc.
- The business-service divisions at both laboratories have limited depth in key positions.
- Significant infrastructure issues create risk for ongoing operations:
  * Fermilab has problems with power-distribution facilities and
  * SLAC has issues with recapitalization of facilities and utility systems.

Charge Item 4

- Both laboratories use external and internal reviews to evaluate performance (although differently).
- However, neither laboratory has a formal benchmarking program.

Charge Item 5

- ES&H staffs at both laboratories have the attention and involvement of senior management.
- ES&H planning and implementation is visible and flows from the top to the bottom of each organization.

He summarized several recommendations common to both laboratories:

- Using a bottoms-up approach, they should extend current manpower analyses through FY09 to determine the required staffing levels and skill mix.
- They should develop plans for infrastructure renewal. (It is a problem at both laboratories.)
- They should institute a formal benchmarking program with other laboratories to assess the efficiency of laboratory operations.

Both laboratories should be commended for their recent successes with large, high-profile projects. Lower-priority activities have been cancelled or modified dramatically (e.g., the detector upgrades at Fermilab and the End Station A program at SLAC). However, significant infrastructure issues present challenges to ongoing operations. The staff is performing heroically, but this level of effort may not be sustainable; moreover, the business-operations staff is thin and stressed.

These laboratory-operations reviews offer a *snapshot*; they are not a validation of laboratory priorities, stated capabilities, or out-year resource plans.

Staffin asked if a Columbia Shuttle type of issue was arising, the staff are being pushed too hard, or was it always this way. Lehman responded that both laboratories have ambitious portfolios. Their business staffs are thin, and infrastructure is aging, as is the workforce. Those observations concerned all on the committees. Both laboratories are lean in all areas.

Witherell said that Fermilab has tightened its belt for several years but there was not much more it could do to cut costs. Management always worries about the age problem. It does not currently have a handle on that problem. The Laboratory's management does not disagree with any of the findings.

Dorfan agreed with Witherell. SLAC has been operating very lean. The Review Committee agreed that, given the budgets and programs the laboratories have, the priorities have been selected very well and the laboratories are coping with challenges well. Where the infrastructure's problems are known, plans have been made to fix them.

Tigner asked what method or metrics would be used to show that the efforts and output are commensurate with the investment. Staffin replied that DOE and NSF believe that these are lean organizations and that they prioritize well. This is a different way of analyzing the laboratories than is usually done. Lehman and the review committees did a very good job. What the payoff will be is still unknown. Lehman commented that this was a first step. It is hoped that the laboratories will do similar self-assessments in the future and that the reviews will be streamlined in the future.

Parriott commented that this was one of the best reviews done for a long while.

Kirk said that HEP will probably arrive at a process that starts with scientific goals, goes through operations, and arrives at judgments of effectiveness. Staffin stated that this review is an attempt to see if what is being funded adds up to the goals set.

Ritz asked how thin "thin" was; that is, how significant the deficit in manpower was. Lehman replied that the committees did not get to that level of detail. Bernthal asked if there are clear implications on the allocation of resources within DOE. Staffin said that it was too early to say; he would want to talk about that issue with the laboratory directors. HEP is looking for trends, common problems, and omissions so it can deal with them.

Montgomery asserted that the laboratories need to attract bright, young mechanical engineers and electrical engineers. Internships and fellowships exist to do that. Peggs added that it is a challenge to keep the stream of physicists going, also.

Gilman introduced **James Whitmore** to report on NSF funding for elementary-particle physics (EPP). He noted that the Directorate for Mathematical and Physical Science (MPS) interacts with several other NSF directorates, such as CISE and Education and Human Resources, and itself has an Office of Multidisciplinary Activities. An update of NSF's EPP budget showed a base of $70 million in FY04, which funded theory, astronomy, accelerator-based physics, and Cornell. The breakdown is

Accelerator-based activities with Cornell: $50.94 million

Particle astrophysics (SPINOFF): $10.83 million

EP–astrophysical theory: $9.03 million

The FY05 request reflects an increase produced by help in funding the LHC as well as advanced planning for RSVP. There is also $40 to 50 million that will come from other activities. Major projects supported include Cornell/CESR (Cornell Electron Storage Ring), LIGO (Laser Interferometer Gravitational Wave Observatory), LHC, RSVP, Ice Cube, and potentially the future underground laboratory.

With the RSVP Project, MREFC (major research equipment and facilities construction) funding will end this year, and operations and maintenance funding will increase starting in FY07. RSVP received $6 million for advance planning in FY04. Implementation funding (including construction funding in FY05) will peak at more than $40 million per year between FY04 and FY08.

With the Underground Science Laboratory, three solicitations have been announced to

1. Develop the scientific and engineering case for the range of potential experiments needing underground access (the elements),
2. Describe the associated technical requirements on the infrastructure and instrumentation, and
3. Group into modules the elements with similar scientific motivation and associated technical requirements for infrastructure.

Currently, the House markup of the FY05 budget request calls for a 2% decrease in the NSF budget from the FY04 funding level; the Senate version calls for a 3% increase.

In summary, NSF is working with many partners to bring added value to EPP projects. In addition, it is entering a new phase in its relationship with DOE.

Chivukula asked why the budget contained a 10% decrease for new core-research funding. Dehmer replied that the NSF has good years and bad years produced by directed funding and initiatives. Core-research grants decreased because funds are committed for 2 to 3 years; current-year grants have to absorb any changes.

Ruchti asked if the FY05 funding for MPS would be sustained. Whitmore replied affirmatively; he added that some directorates are funded year by year.

Hewett suggested that they try to even out the rate of funding for new grants. Dehmer responded that they try to do that, but sometimes life is unfair. He said that they will try to avoid such fluctuations in the future. With the emphases on centers, initiatives, and facilities, the theory program has been under enormous stress. Support for theory should be strengthened. MPS will have a workshop this fall. The number of new faculty members in cosmology and theoretical particle physics, combined with the tendency for the budget to be flat or slightly worse, has created a pressure point that must be addressed. Priorities have to be set, and the best investments for the field have to be made. Theory is one such area that needs addressing. He did not want the core programs to take on any more pressure. That core program, the great discovery machine, has to be treated as a critical asset.

Turner noted that there have been problems funding theory. The workshop referred to by Dehmer will address that issue. Funding of research groups at NSF is extremely competitive. When NSF funds a grant, it gives people what they ask for. It does not cut back the level of funding over the term. In DOE's HEP, renewals can be at reduced levels of funding. The custom is to continually support these groups but at a lower rate.

Gilman introduced **Glen Crawford** to speak on the DOE HEP budget status.

Essentially, FY04 is done. Significant new items were started in FY04 since the prior HEPAP meeting: the QCDOC (quantum chromodynamics on a chip) 5-teraflop (TFlop) Prototype [with the DOE offices of Nuclear Physics (NP) and Advanced Scientific Computing Research (ASCR)] at Brookhaven National Laboratory (BNL), the BaBar instrumented flux return (IFR) chambers upgrade was approved and will be done next year, the BTeV R&D phase was approved, the next generation of the AXION II experiment was begun at Lawrence Livermore National Laboratory (LLNL), the high-energy physics demographic survey got under way [being carried out by Lawrence Berkeley National Laboratory (LBNL) plus university people], and the NAS study of high-energy physics was launched. DOE funding was completed in FY04 for the Cryogenic Dark Matter Search (CDMS) and Auger.

The final numbers showed that the total HEP budget for FY03 of $717.9 million went up by about 2.2% in FY04. This was a good budget year, with several small, new projects getting started and others being successfully completed. There was continued excellent performance of PEP-II (the Positron Electron Project) and Tevatron and implemented upgrades.

The House passed the FY05 Energy and Water Bill in late June with an increase of $16 million for HEP. The Senate has not acted on it; debate was originally scheduled for July and has been deferred and not rescheduled. A continuing resolution appears likely. The DOE budget has historically been signed in December to late February.

What does a continuing resolution mean? Continuing contracts and grants will be funded at FY04 levels. Usually, the DOE chief financial officer releases one-twelfth of the funds each month until the continuing resolution is lifted. No new projects are started. Such a resolution will affect ongoing projects with significant procurements (e.g., the Gamma Ray Large Area Space Telescope, or GLAST) or new projects trying to ramp-up (e.g., BTeV).

A bar chart of the HEP funding history showed a long, slow decline in deflated dollars from 1996 to 2005. FY05 will be a tight budget year (+0.5% growth) unless Congress acts. The only new project on the table is BTeV. Several others are looking at successful completion in FY05 [GLAST, NeUtrinos at the Main Injector/ Main Injector Neutrino Oscillation Search (NuMI/MINOS), LHC Accelerator, and Run IIb detectors]. Unfortunately, these do not "free up" nearly enough resources in the out-years if budgets stay flat.

The FY06 budget is with the Office of Management and Budget (OMB). The general guidelines from OMB and the Office of Science and Technology Policy (OSTP) are not to expect new dollars. The priorities of the budget are Homeland Security R&D, networking and information technology R&D, nanotechnology, physical sciences, biology of complex systems, climate, and water and hydrogen R&D. There are also less favorable analyses. At least at DOE, there is increased emphasis on out-year planning and profiles, producing a consistent, long-term plan. The LHC research program is an example of long-term planning of funding for FY01 through FY08.

The *Facilities for the Future of Science: A Twenty-Year Outlook* is the statement of priorities for big-ticket items. For smaller items ($2 million < total estimated cost <$50 million), we have more flexibility, but the budget process still requires that these be identified well in advance. DOE began developing its FY06 budget request in March.

In FY05, less than 5% of the HEP budget is spent on new construction and major items of equipment. This is not a healthy trend for the future, but the burden in the near-term is on facilities operations.

The nominal process begins two years before the fiscal year with congressional hearings, which are followed by a preliminary, internal draft budget request; an initial draft budget request; a final budget request; an initial financial plan; submission to OMB; OMB hearings; a revised financial plan; OMB passback; submission of the budget request to Congress; and congressional rollout.

In summary, FY04 was good. FY05 will likely be challenging and not likely as good as FY04. Priority decisions will need to be made in a resource-limited environment. Even "small" new projects (>$2 million) need more advance planning than in the past.

Dragt asked what effect the completion of the Spallation Neutron Source (SNS) will have. Crawford replied that that is up to Ray Orbach and his priorities, as stated in the long-term facilities plan.

Ruchti asked what the Small Business Innovation Research (SBIR) and Small Business Technology Transfer Program (STTR) funding levels are expected to be. Crawford replied, $17 million in FY05 and about the same in FY06.

Gilman introduced **Abraham Seiden** to discuss the P5 (Particle Physics Project Prioritization Panel) letter.

P5 was asked to evaluate the latest BTeV plan for detector completion. The Department of Energy granted Critical Decision 0 (CD-0, the Statement of Mission Need) to BTeV in February 2004. The Office of Science then conducted a technical, cost, and schedule review chaired by Daniel Lehman in April 2004. The conclusion of that review was that the technical scope was adequate to meet the science requirements and that the cost estimate was fairly complete and reliable. However, the presented schedule, "Detector Complete and Ready for Commissioning with Beam by October 2009," was judged to be not achievable with the proposed funding and resource profile. Fermilab and the BTeV project are preparing a new schedule that may complete the detector in stages. That was done. Some elements will be completed in FY09, and the rest in FY10. The new schedule with a staged plan was found credible by the Lehman review. The Fermilab Physics Advisory Committee also took this up, and this Panel's information was supplied to them.

One year ago, it was believed that heavy-flavor decays are sensitive to new weak-scale physics, which can contribute through loop and box diagrams in the B sector. As such, these decays provide physics probes that are supportive of the LHC physics program, which directly looks for new particles at this scale. With the International Linear Collider having an earliest start date of 2015, heavy-flavor decays may be the main supporting program of LHC physics through the first half of the next decade.

Today, BaBar and Belle together provide about 500 fb^{-1} of data. $\sin 2\beta = 0.726 \pm 0.037$ in tree diagrams (compatible with expectations); $\sin 2\beta = 0.42 \pm 0.08$ in $b \rightarrow s$ Penguin diagrams (different by a few σ). $\sigma_\beta = 1.6°$, $\sigma_\alpha = 11°$, and $\sigma_\gamma = 19°$. For the latter two angles, the error is what is expected. It may take another 3 years to see if there is a discrepancy or not.

To summarize what has happened, P5 wrote to the agencies: On September 29, 2003, the High Energy Physics Advisory Panel (HEPAP) endorsed the Particle Physics Project Prioritization Panel (P5) Report recommending that BTeV move forward to a construction start, subject to overall budget constraints within the program, and with a goal that the construction be completed by the end of FY 2009. P5 recommended the special optics for the interaction region. This recommendation has since been adopted as part of the project. The recommendation on schedule was primarily to keep BTeV competitive with LHCb on topics where they compete directly, although for a number of topics BTeV will be clearly superior.

This past year, the BTeV project has started along the path required for the funding agencies to make a final decision on whether to begin construction. The recent CD-1 review, chaired by Daniel Lehman, examined the potential cost, technical status, and schedule for BTeV construction. Although the cost and technical status were deemed to be very credible for this stage of a project, a scheduled completion date by FY 2009 was seen as too aggressive given the likely timeline for funding. To ameliorate the schedule concerns, BTeV has devised a plan whereby the detector completion is staged, allowing an extra year for the funding profile.

The Lehman committee examined this new schedule and found it to be credible, with sufficient float for the various construction activities. The choices for staging were made such that, in 2009, the first-stage detector would be fully competitive with LHCb in physics areas of common overlap, as was felt to be desirable in P5's report of the previous year, with the complete detector in 2010 allowing BTeV to fully exploit the physics areas in which it has unique capabilities. Staffin and Dehmer's recent letter to P5 asks the Panel to evaluate this physics plan and, in particular, whether it is consistent with the expectations on which the P5 report of the previous year was based.

P5 convened a meeting on July 21 and 22, 2004, to evaluate the physics implications of the BTeV staging plan. The meeting was primarily focused on a general discussion of B physics. P5 looked at the impact of new results from the past year, the expected evolution of results from the B factories by 2009, as well as talks from the BTeV spokespersons and the spokesman for LHCb. It also heard from the chair of the Fermilab Physics Advisory Committee (PAC), who described the PAC's independent assessment of the BTeV plan, and Fermilab Director Witherell, who presented the Fermilab program for the next few years and how the construction of BTeV would fit into this program. The Panel noted that, prior to its meeting, the Fermilab PAC provided an independent, strong, unanimous endorsement of the BTeV staging plan.

BTeV will potentially be the flagship experiment in the United States in the quark- flavor physics area after the LHC startup. In evaluating BTeV, the primary criterion remains that it be the world's best experiment to look at the imprint of new physics found at the LHC in the B^0 and B_s systems, providing a tool to decipher the character of that new physics. BTeV would also provide a broad and deep program of quark-flavor physics, with access to the meson and baryon states of both the bottom and charm quark systems.

The Panel examined the potential physics output of BTeV approximately 10 years from now, particularly in the area of those weak B decays that are unambiguous tests of the underlying weak-scale physics. It also examined potential contributions from the two B-factories and the expectations for LHCb. The B-factory numbers were based on extrapolating results known today to a combined luminosity for both B factories of 3 ab^{-1}, the integrated luminosity that could be expected by the end of 2009. We assumed that the results from the two B-factories could be averaged. The results for BTeV and LHCb are based on their Monte Carlo calculations and integrated luminosities of 9 and 6 fb^{-1}, respectively.

The results of the above comparisons can be roughly summarized as follows. BTeV will provide an improvement of typically a factor of 2 in the errors relative to having both the B-factories and LHCb running on a broad set of measurements of the unitarity triangle angles in the Standard Model for the B^0 system if the B-factories accumulate no more than 3 $ab^{-1}$of data. For the B_s, which is not produced at the B-factories, BTeV's errors on the unitarity triangle angles are smaller than those of LHCb by roughly a factor of 3.

In terms of the number of events, BTeV would provide a factor of 12 more B decays written to tape than a 3-ab^{-1} B-factory data sample and a factor of 6 more than LHCb. The tagging efficiency of data on tape is typically expected to be a factor of 3 lower for BTeV compared with that of the B-factories, and reconstruction efficiencies for the different experiments are channel dependent. For decays involving leptons, the BTeV and LHCb data sets would be comparable because of the specific lepton trigger of LHCb. These general data sets will be particularly useful when looking for forbidden or suppressed decays, which may not require tagging or, at specific decay channels, may be found to be of interest after the LHC turn-on.

In conclusion, given the Panel's analysis, the conclusions of the previous year are unchanged in the staging scenario proposed by BTeV, and those conclusions are reaffirmed. The method of staging chosen by BTeV is an appropriate choice to maximize its physics opportunities. The Panel is not concerned about the comparison of the two machines.

However, it does have a concern that the staging scenario stretches the BTeV schedule as far as can be supported. If various constraints, budget or technical, would result in a completion date beyond the end of FY 2010, the Panel would not support a start of the project. There are two reasons for this conclusion. The first is that the physics competitiveness of BTeV would be compromised if it begins too many years after the LHC startup. In particular, a start after LHC is fully commissioned and running smoothly for several years will allow LHCb to extensively examine the B_s system, which is the unique physics area of the experiments at the hadron colliders.

A second concern is that the United States has an exciting agenda of other projects outlined in the Roadmap, with new projects being invented as more is learned about the universe. These projects compete for funds and require timely support. So, the stretching out of projects, including the additional cost for running accelerators, delays the onset of these other exciting projects. The Panel is worried about opportunity costs. Finally, the BTeV capital and operating costs presented to the Panel are somewhat higher than had been expected, based on last year's studies. This is the case even after accounting for the new infrared (IR) optics and the need for the support of future projects; the Panel would not like to see additional cost increases for BTeV in the future.

Kim said that she understood that the cost estimate went up significantly. Seiden replied affirmatively: about 20%. It did not have a Lehman review before. Witherell stated that the cost went up 1%. Butler said that what went up 20% was the "Total Project Cost," the TPC, which includes R&D already completed, R&D in the FY05 before formal project start, a small amount of R&D in FY06, and some operating costs that were not considered part of the project cost, the "Total Estimated Cost" or TEC. Also, the cost referred to is in "then-year" dollars and includes inflation. The actual change in cost to DOE is very small.

Langacker asked if this situation was just a horse race or whether there was a reason one is wanted faster. Seiden replied that BTeV would be better than LHC, although LHCb could upgrade. It is a judgment call of the Committee.

Oddone asked about the implication of a limited schedule. Seiden said that they could be done sooner if there were more funding. The crystals are not a critical issue.

Stone noted that the numbers the project was presented with originally were cut by 40% in FY05. If it had that $9.5 million, it could have completed the project earlier.

Gilman recessed the panel for lunch at 12:37 pm.

Thursday Afternoon

The meeting was called back into session at 2:04 pm, and **Barry Barish** was asked to present the report of the International Technology Recommendation Panel (ITRP).

Two parallel developments have occurred during the past few years:

1. The science, represented by precision information from the Large Electron-Positron storage ring (LEP) collider, LHC/ILC complementarity, and Higgs mass must have spin 0. The quantum numbers are what the ILC is all about. At LHC one can tell how many energy levels there are.
2. Design and technology has proceeded through R&D. The International Linear Collider Technology Review Committee's second report validates the readiness of L-band and X-band concepts and points out what else needs to be pursued.

Why decide the technology now? There currently is an embarrassment of riches. There are two alternatives (warm and cold). The concepts are well understood, and there are no show-stoppers. However, R&D is expensive; costs need to be limited by focusing on one technology, a final global design. That is the cold technology. A construction decision must be made in about 5 years.

The Panel started in January and was charged to finish this calendar year. It has held six meetings to survey the technology and to deliberate about the pros and cons of each.

The process involved studying a lot of material, making site visits, hearing presentations on other technologies [C-band and Compact Linear Collider (CLIC) technologies], interacting with the ILC community, developing sets of evaluation criteria (having each proponent answer a related set of questions), and assigning a lot of internal homework.

The charge was to recommend an ILC technology, choosing between two specific designs. The Panel tried to evaluate a criteria matrix that had six general categories:

1. scope and parameters specified by the International Linear Collider Steering Committee (ILCSC);
2. technical issues;
3. cost issues (construction and operating costs and relative costs);
4. schedule issues (within 5 years);
5. physics-operation issues (energy resolution); and
6. more-general considerations that reflect the impact of the ILC on science, technology, and society.

The terms dictated by Category 1 call for 200 to 500 GeV, acquiring 500 fb^{-1} in 4 years upgradeable to 1 TeV with a luminosity sufficient to acquire 1 ab^{-1} in an additional 3 to 4 years. The Panel evaluated each of these categories partly by questioning proponents. Either technology could meet this category's goals, although luminosity goals were aggressive (but cold technology had a better chance at achieving a stable beam).

In terms of Category 2, CLIC technology was impressive, although it will face many challenges. C-band progress was gratifying. X-band and L-band linacs and subsystems were also investigated. R&D is far advanced. (For warm technology, major subsystems have been built to study actual performance, and subsystem designs are further advanced than those for cold technology.) The accelerating structures have risks deemed comparable in the two technologies. For the cold technology, industrialization is advanced; many of these components will be tested in the next 5 years. A superconducting linac has a high intrinsic efficiency for beam acceleration. The lower acceleration gradient in superconductor cavities implies that the length of the main linac would be larger than for a warm machine of the same energy. In a superconducting rf structure, the rf pulse lengths, length of bunch trains, and inter-bunch time intervals are all large (which is good). But the complex and very long damping rings and large heat load on the production target for a conventional positron source might require a novel design. Achieving design luminosity will be easier with the cold technology. Therefore, the Panel deems the cold machine more robust.

In investigating Category 3, the Committee collected cost data and estimates and found a lot of uncertainties at the pre-industrial stage of development. There are a lot of complications affecting warm- and cold-technology costs (construction, learning curves, industrialization methods, contingencies, etc.). The Panel concluded that the costs of warm and cold technologies are similar.

In terms of Category 4 (getting to a design by 2010), nontechnical issues will drive the progress.

In terms of Category 5, in a warm machine, the pileup of energy from multibunch crossings will be a potential problem. The energy spread will be smaller in a cold machine. A cold machine offers more choices.

In terms of Category 6, ILC R&D will affect other scientific areas (materials science, light sources, etc.). New light sources and X-ray free-electron lasers (XFELs) will open new opportunities in biology and materials science.

The Panel recommends the Linear Collider be based on superconducting rf technology. This action recommends a technology, not a design.

The good features of cold technology are

- reduced complexity,
- possible enablement of increased beam current,
- reduced sensitivity to ground motion,
- lower risks,
- progress in industrialization,
- expected conduct of prototypes, and
- lower power consumption.

This recommendation was made to the ILCSC and to the International Committee on Future Accelerators (ICFA). ICFA unanimously endorsed the recommendation. It was also endorsed by the Funding Agencies for the Linear Collider (FALC).

A new global design will be developed. There is a need to capitalize on experience already gained and to pool experience resources. The final design will be coordinated by an Intermediate Central Design Team, the first collaborative meeting of which will be held in November.

Hewett commended the Panel for its timely response.

Dehmer stated that the operating cost of superconducting rf is clearly lower. Barish agreed that the power costs would be less.

Staffin extended congratulations to Barish and to the Panel, stating that this recommendation reflects the will of the world community. A management challenge arises in realigning the national plan. The budget will be a constraint as will be the cap on DOE's funding. As of 2005, the apportionment will be lifted. What is needed is to move forward with an international design plan and to point out the applications outside particle physics.

Maury Tigner was introduced to speak about the next steps for the International Linear Collider (ILC).

A place is needed for the Central Team, so a group in has been assembled to ask who would host such a team. An evaluation committee of six was appointed in August; a chair is yet to be appointed. Nine offers have been received [KEK (the High Energy Accelerator Research Organization in Ibaraki, Japan), LBNL, SLAC, TRIUMF (TRI-University Meson Facility in Vancouver, BC), FNAL, BNL, CORNELL, RAL (Rutherford Appleton Laboratory, offering two sites), and DESY (Deutsches Elektronen-Synchrotron)]. A director for the Central Team must also be recruited. A search committee was appointed in August, but a chair is yet to be appointed.

A memorandum of understanding (MOU) must be ratified under which the "big" laboratories will express, at least for the beginning, their obligations to carry forward the initial work of the ILC design, planning, etc. An initial draft was circulated and discussed in August. The revised draft will be discussed at the KEK meeting on November 16.

Interim activities will complement the work of the central committee. An ILC Workshop will be held for accelerator experts on November 13-15 at KEK to analyze what the status is and to draw up a list of things to do. The question will be raised, what possible follow-on activities can be carried out at the LICSC meeting in November.

It is hoped that the Director and Central Team location will have been decided on at or before the next formal ICFA meeting in February of 2005. The Director Search Committee is made up of two representatives from each region, as is the Site Evaluation Committee.

The work is going to have to be done in the field. The Global Design Center is a coordinating effort to bring together the work of the three regional teams (United States, Europe, and Asia, each with a regional director). The Global Design Center will be small, about 10 people.

Krull asked if the Global Design Center could be one of the regional centers. Tigner replied that that depends on who the director is, whether the center is located at a potential site of the ILC, visa problems, etc.

Strauss noted that this organization looks like ITER (the International Thermonuclear Experimental Reactor) and asked how it is different. Tigner responded that this organizational concept is not far enough along to say it is like ITER. Also, the community has agreed to accept the Panel's recommendations and authority.

Gilman introduced **Robin Staffin** to report on the London meetings of FALC. The most recent meeting endorsed the technology selection and the establishment of a working committee. At that meeting, Barish made the recommendation of the ITRP. Witherell reported that the major laboratories had accepted the technology recommendation and endorsed the concept of a coordinating group. A group was set up to prepare arrangements for funding for an ILC. The working group is to meet before the end of 2004.

Montgomery asked if a list of countries that are interested in an ILC was available and why a working group was being established. Staffin replied that a list had been compiled from collective experience. A working group will be smaller and can meet more often and respond to developments more quickly. Montgomery offered the opinion that, if one goes to a subset, one relinquishes the global common ownership. Staffin replied that no agreement was entered to yield any authority to a subcommittee.

Dorfan noted that he had to have a site and director by February and asked if Staffin wanted the naming of these done by the full Committee. Staffin said that he would think about it.

Langacker asked what countries were involved in these meetings. Staffin recited a list.

Ruchti questioned whether there is a procedure in DOE to shift the other technologies (e.g., X-band) to other organizations. Dorfan said that it had already been done. Kim asked if that meant that SLAC would continue X-band work. Dorfan said that SLAC's primary efforts will go into the ILC; its efforts in X-band will also be continued, and there are commonalities that can be exploited.

Rosen queried whether other communities (like Jefferson Lab) would be drawn into this effort. For example, will Fermilab, which has been working on rf technology for a long time and has partnered with many other organizations, be involved? Staffin replied that a workshop on SC technology will be held for that community in the near future.

Gilman asked about university R&D efforts in accelerators and detectors. Staffin replied that an increased apportionment has been requested to fund R&D at universities. DOE will coordinate proposal reviews and awards with other agencies to fund university ILC R&D.

Ruchti asked whether, if one sent in a proposal to one agency, it would be reviewed by both DOE and NSF. Dehmer responded that the details have not been worked out, but the proposals will be reviewed by a joint review panel. NSF has committed a small amount of money for FY05 that will be stable and can be increased in subsequent years. The money will be for universities, and proposals will be co-reviewed.

Dorfan asked if this funding was for detectors and accelerators. Staffin replied, yes.

Baker noted that, previously, a university that was part of a consortium had a better chance of funding and asked if that advantage would continue in ILC R&D. Dorfan commented that a professor at a university should interact with the ILC community first. There will not be one giant proposal, but funding will be focused on ILC R&D.

Mallik called attention to the fact that the ILC student group had advised the panel and asked if that would continue. Dehmer replied, yes; it is valuable, but it will not replace independent peer reviews.

Kim asked if there would be one review panel or one DOE and one NSF review panel. Dehmer replied that there would not be a joint solicitation, but there would be one set of review panels that will report their findings to both NSF and DOE.

Butler asked if there would be a solicitation that describes what is being looked for. Dehmer responded, no, but there will be rules defined. Butler asked if a scale of funding had been decided. Dehmer said that NSF will devote $200,000 next year, and Staffin said that DOE would put up $400,000 in accelerator R&D and $700,000 in detector R&D. Krull asked if the funds would be apportioned separately in that ratio. Dehmer did not think that a fixed ratio is reasonable. Staffin said bluntly that there is no fixed ratio.

Ritz noted that evaluation criteria will be difficult to determine. Dehmer said that no one had figured out how to do that, but they would.

Kim asked what was going on in each laboratory. Dorfan said that SLAC had said that they would support whatever technology was selected and would persist with that commitment. SLAC will contribute what it can to this design. First, one must figure out what needs reconsideration and what current devices can be adapted. People want

Tigner noted that, on the superconducting rf front, an attempt is being made to achieve higher gradients through reshaping techniques. Damping rings are miserable. That will be the major ILC contribution.

Kirk said that BNL had been a member of the collaboration for many years and has contributed in theory and in SC magnets. It intends and desires to contribute to ILC, but it does not have the wherewithal.

Rosen asked if any of the U.S. laboratories were moving toward industrialization. Witherell replied that all the laboratories are trying to build up a U.S. capability. Dorfan said that the klystron is being done at Communications and Power Industries (CPI). Oddone said that LBL has been closely allied with SLAC and has been working on damping rings. Strauss noted that Ni-Ti alloy production has been quadrupled and was a uniquely U.S. capability.

Price said that ANL has a continuing interest that will be turned to this purpose.

A break was declared at 4:08 p.m. The meeting was called back into session at 4:30 p.m., and **Sally Dawson** was asked to report on the EPP2010 study. This study on elementary particle physics is being carried out under the auspices of the Bureau of Physics and Astronomy of the National Research Council, and Burton Richter is the Chair. It surveys all branches of physics every 10 years (and hence is called the Decadal Survey), covering

- Atomic, molecular, and optical science;
- Plasma physics;
- Condensed matter and materials physics;
- Elementary particle physics;
- Nuclear physics; and
- Gravitational physics.

The most recent Decadal Survey, *Physics in a New Era* (2001), was chaired by Tom Appelquist. EPP (98) was chaired by Bruce Winstein.

The Decadal Survey has two purposes. The first is informational. It provides a snapshot of the field that is useful for tracking and understanding the evolution of the science. It also provides a process whereby emerging opportunities can be identified and developed, and all the fields will focus on identifying the science drivers for the physics and the enablers of progress towards science goals.

The EPP2010 Committee is charged to identify, articulate, and prioritize the scientific questions and opportunities that define elementary particle physics. The emphasis of the charge is on ranking science priorities, not the facilities or instruments necessary to do the work. It will look beyond particle physics and recommend a 15-year implementation plan with realistic, ordered priorities to realize these opportunities.

The study will build on many earlier studies:

- *Quantum Universe* (P. Drell; just out)
- *The Science Ahead, the Way to Discovery* (Bagger/Barish)
- *The Physics of the Universe* (OSTP)
- *Facilities for the Future of Science: A Twenty-Year Outlook* (Orbach)
- *Quarks to the Cosmos* (Turner)

She listed the Committee members, commenting that it is not one's usual committee. It has many nonphysicists, including an expert on federal budgets and one on scientific ethics. Its major goals are to engage other communities, sharpen physics questions, place U.S. particle physics in a broader context and in an international setting, and present a compelling vision of the science of particle physics.

Its first meeting will be held in Washington, D.C., on November 30 to December 1, 2004. It will focus on science questions and general issues related to how to set scientific priorities. The ultimate goal is to write a report by December 2005.

The committee is interested in hearing from the community. There will be an open forum at every meeting organized by the Division of Particles and Fields (DPF) Executive Committee. Suggestions can be sent to epp2010@nas.edu.

In conclusion, particle physics has an exciting story to tell, and EPP2010 is an opportunity to communicate the science.

Ritz commented that this survey is great and has a lot of potential. The key is that the community takes ownership and has buy-in. One component is to educate the non-particle physicists. Another is to draw them in and get them to lend their expertise. He asked how the Committee was going to sample all the subsections of the particle physics community. Dawson replied that that was why the Committee is going to SLAC and Fermilab. Ritz asked if they had thought about topics. Dawson responded, yes; the talks will be selected to cover all bases.

Baker asked if HEPAP would be asked to approve this report. Dawson responded, no, it is an National Research Council (NRC) report. Staffin stated that this was one value of the broad disciplinary authorship. He said that this study will be an expression of science as human culture and that he was looking forward to it.

Rosen asked if the Committee had thought about having people in other disciplines talk about the impact of high-energy physics. Dawson stated that that would be a good thing to do.

Kirk commented that the difference between this study and *Quarks and the Cosmos* is that the earlier report was an interface between disciplines. This study is billed as outreach. Dawson said that the Committee is going to define EPP to include the edges of EPP. Kim asked what the boundaries of this inquiry are. Dawson responded that the Committee will define the boundaries as it goes along. The charge is to define EPP. The Committee will start with other assessments and build on them.

Ritz noted that the customer is not just the policymakers but the physics community, also. That is why organized community involvement is needed. How does one draw a line between the science desired and the facilities needed? One could run into problems with what capabilities are available. Dawson pointed out that that is why the word "realistic" is in the charge. Dehmer said that it may come to two phases: prioritization will require a list of topics, and then a 15-year implementation plan, which will require considering the need for facilities.

Ferbel said that he could not imagine that scientists in the field will not try to impress people that the ILC is the way to go, a position developed over the past 3 years. Dawson said that she could not see this Committee rubber stamping anything. Meyer said that he would not call it a rubber stamp. They might say, "This is interesting science, but it will cost a lot of money."

Gilman introduced **Boris Kayser** to update the Panel on the American Physical Society (APS) Multidivisional Neutrino Study.

The past six years has produced compelling evidence that neutrinos have mass and mixing. However, there are open questions about the neutrino world, so there is a need for a coherent strategy for answering them. A year-long study of the future of neutrino physics has been sponsored by the APS divisions of Nuclear Physics, Particles and Fields, Astrophysics, and Physics of Beams to address that need. The primary purpose of that study is to move toward a coherent strategy for answering the open neutrino questions, a clear, unified plan that funding sources can easily consider and promote. The study will lay scientific groundwork for the choices that must be made during the next few years. Its purpose is not to choose between techniques but to select scientific bases.

A grassroots study like this, cosponsored by several APS divisions, is unprecedented. Importantly, it aims at consensus, which is not a trivial goal. A secondary but important purpose of the study is to explain to colleagues in other areas of physics, to funding sources, and to the general public why neutrino physics is now so exciting. One can find information about the study at the website: www.interactions.org/neutrinostudy.

The study has two cochairman, an organizing committee, and several working groups:

- Solar and Atmospheric Neutrino Experiments,
- Reactor Neutrino Experiments,
- Superbeam Experiments and Development,
- Neutrino Factory and Beta Beam Experiments and Development,
- Neutrinoless Double Beta Decay and Direct Searches for Neutrino Mass, and
- What Cosmology/Astrophysics and Neutrino Physics Can Teach Each Other.

It also has a theory discussion group.

The Organizing Committee plus the working-group leaders form the Coordinating Committee that guides the study. The final study report is being written by the Writing Committee, which submits its work to the Coordinating Committee. The working groups have done a great deal of work, mostly separately but sometimes in concert. They have summarized their findings in their working-group reports. A general meeting was held in Snowmass so everyone could see what all the subcommittees were doing and so everyone could voice an opinion. With the working-group findings and the Snowmass discussion as input, the Writing Committee has been working on the cross-cutting heart of the study's final report. The final report has been submitted to the Coordinating Committee for comment. The thrust of most of the major conclusions is fairly clear.

We do not know how many neutrino mass eigen states there are. If the Liquid Scintillator Neutrino Detector (LSND) experiment is confirmed, there are more than three. If LSND is not confirmed, nature may contain only three neutrinos. Then, from the existing data, the neutrino spectrum would be normal (with a low-mass solar pair and a high-mass atmospheric singlet) or inverted (with a high-mass solar pair and a low-mass atmospheric singlet). Generically, grand unified models predict the normal scheme. The inverted scheme is unquarklike and would probably suggest that there is a lepton symmetry with no quark analogue.

When a neutrino of definite mass ν_i interacts in a detector and creates a charged lepton, the latter may be of electron, muon, or tau flavor. The probability that it will be of flavor α is $U_{\alpha i}$, where U is the leptonic mixing matrix. The matrix elements $U_{\alpha i}$ are described by mixing angles and charge-conjugation–parity-violating [CP violating] phases. The solar mixing angle θ_{12} is known with some precision to be 32°. The atmospheric mixing angle θ_{23} lies in the range of 35 to 55°. The third mixing angle θ_{13} is known only to be smaller than 15°. The phase δ, if present, will lead to CP-violating differences between the probabilities for neutrino and antineutrino oscillations. The size of these differences will depend on the value of θ_{13}.

The study participants framed the open questions in three themes. The first is neutrinos and the new paradigm:

- What are the masses of the neutrinos?
- What is the pattern of mixing among the different types of neutrinos (and is there an underlying symmetry)?
- Are neutrinos their own antiparticles?
- Do neutrinos violate the symmetry CP?

The discovery that neutrinos have large mixing angles was a great surprise. Indeed, neutrino physics has been marked by surprises, so the second theme is neutrinos and the unexpected:

- Are there "sterile" neutrinos?
- Do neutrinos have unexpected or exotic properties (e.g., visibly rapid decays)?
- What can neutrinos tell us about the models of new physics beyond the Standard Model (SM)?

The third theme, neutrinos and the cosmos, reflects the connection between neutrinos and astrophysics:

- What is the role of neutrinos in shaping the universe?
- Is CP violation by neutrinos (as opposed to CP violation by quarks) the key to understanding the matter-antimatter asymmetry of the universe?
- What can neutrinos reveal about the deep interior of the Earth and Sun and about supernovae and other ultrahigh-energy astrophysical phenomena?

The conclusions of the study are still under discussion, but a fairly broad consensus has emerged on a number of issues. The study's recommendations for a strong future program are predicated on fully capitalizing on our investments in the current neutrino program:

- Accelerator neutrino experiments within the United States and
- American participation in experiments offshore.

In addition, some of the particularly important future experiments require suitable underground facilities, so having these facilities will be crucial.

Turning to the proto-recommendations for future experiments, a high priority should be attached to a phased program of searches for neutrinoless double beta decay ($0\nu\beta\beta$). Observation of $0\nu\beta\beta$ would establish that

- Lepton number L is not conserved. Then nothing distinguishes an antineutrino from a neutrino.
- Therefore, neutrinos are Majorana particles (i.e., an antineutrino equals a neutrino).
- Nature (but not the SM) contains Majorana neutrino masses.

Many theorists think L is not conserved. The SM is defined by the fields it contains, its symmetries (notably electroweak isospin invariance), and its renormalizability. Anything allowed by the symmetries occurs. The SM contains no ν_R field, only ν_L, and no neutrino mass. This model conserves L. We can try to extend it to accommodate neutrino mass without violating L conservation by adding a Dirac, L-conserving mass term. However, to add such a term, we have to add ν_R to the SM. But, unlike ν_L, ν_R carries no electroweak isospin. Thus, no SM symmetry prevents the occurrence of a Majorana mass term containing ν_R and its charge conjugate. Such a term mixes neutrinos and antineutrinos in clear violation of L conservation. Thus, the ν_i will be Majorana particles.

The rate for $0\nu\beta\beta$ is proportional to the square of an effective neutrino mass,$m_{\beta\beta}$. One can design a phased $0\nu\beta\beta$ program addressing three possible $m_{\beta\beta}$ ranges: The first phase, with a range of 100 to 500 MeV and covering a quasi-degenerate spectrum, would require 200 kg of decaying mass; its status is close. If it is seen, great; if not, one would go on to the next phase. This second phase, with a range of 20 to 50 MeV and covering an inverted spectrum, would require one ton of decaying mass; this has been proposed. If the spectrum is not inverted, the third phase, with a range of 2 to 5 MeV and covering essentially any spectrum, would require 100 tons of decaying mass; this appears to depend on future technology. In the first two phases, more than one experiment is desirable (worldwide) both to permit confirmation and to explore the underlying physics.

The study group would also attach a high priority to a comprehensive U.S. program to complete our understanding of neutrino mixing, determine whether the neutrino mass spectrum is normal or inverted, and search for CP violation in neutrino oscillations. The first component of such a program would be an expeditiously deployed reactor experiment with sensitivity down to $\sin^2 2\theta_{13} = 0.01$. The size of CP violation and the ability to tell whether the mass spectrum is normal or inverted both depend on θ_{13}. Roughly speaking, if $\sin^2 2\theta_{13} < 0.01$, a neutrino factory will be needed to study both of these issues. A relatively modest-scale reactor experiment can cleanly determine whether $\sin^2 2\theta_{13} > 0.01$ and measure it if it is.

The second component would be a timely accelerator experiment with the possibility of determining the character of the mass hierarchy (normal or inverted). An accelerator neutrino experiment can probe several neutrino properties. But only the United States can have baselines long enough to probe whether the spectrum is normal or inverted. Thus, the United States could make a unique contribution. (long baselines are needed to determine the character of the hierarchy because the determination exploits a matter effect that grows with neutrino energy. Thus, working with higher energy is preferable. But then, to keep the small oscillation probability at its peak, one needs to work with a longer baseline as well.)

The third component would be a megawatt-class proton driver creating a neutrino superbeam aimed at a suitably large detector, with the combination capable of observing CP violation. Such facilities are needed to be able to determine whether the spectrum is normal or inverted, and to observe CP violation, for essentially any $\sin^2 2\theta_{13} >$ 0.01 or 0.02.

Why would CP violation in neutrino oscillation be interesting? In the see-saw explanation of why neutrino masses are so tiny, each familiar light neutrino has a very heavy neutrino partner. These heavy neutrinos would have been made in a hot big bang. If oscillations of the light neutrinos violate CP, then quite likely so do the decays of the heavy neutrinos. As a result, in the early universe, these decays would have produced unequal numbers of leptons and antileptons. Perhaps this inequality was the original source of the present preponderance of matter over antimatter in the universe.

An experiment that could measure the energy spectrum of the proton-proton-fusion (pp) solar neutrinos would be important. The pp neutrinos are almost *all* the solar neutrinos. By studying these low-energy neutrinos, one could confirm the Mikheyev-Smirnov-Wolfenstein explanation of solar-neutrino behavior, and test whether the pp fusion chain is the *only* source of solar energy.

Looking ahead, a neutrino factory (or beta beam) is the ultimate tool in neutrino physics. It may be the only way to study CP violation and other issues. Substantial neutrino-factory R&D is needed if this facility is to be possible in the long term. Moreover, worldwide, at least two detectors sensitive to a galactic supernova should be operational at any given moment.

Montgomery said that he was worried that the language employed might make it undoable by pricing oneself out of the game. Kayser replied that one is not going to get to 0.01 anytime soon. But one needs to start down the road.

Kroll asked which of the things Kayser had pointed to require an underground, background-reducing facility. Kayser cited as examples the search for neutrinoless double-beta decay, the study of the low-energy solar neutrinos, and the accelerator neutrino experiments involving a very large detector, if that detector is of the multipurpose type that can also search for proton decay.

Baker asked if sterile neutrinos could be a component of hot dark matter. Kayser responded that there is strong evidence that most dark matter is cold dark matter, so even if there are sterile neutrinos in hot dark matter, one has not solved the dark-matter problem.

Langacker noted that there are some that believe neutrinos are not of Majorana character. Kayser indicated that it is indeed possible that neutrinos are not Majorana particles, but he and Langacker agreed that the majority of theorists do hold the prejudice that neutrinos *are* Majorana particles.

Kim asked about the optimum range of L, the distance that neutrinos travel in a long-baseline experiment.

Garth Illingworth was asked to report on the Astronomy and Astrophysics Advisory Committee (AAAC). The AAAC is a new advisory committee whose reports should move in the same direction and use the same language as those of HEPAP. This advisory committee grew out of OMB (and congressional) interest in optimizing the return on astronomy investment, minimizing duplication, putting all astronomy under one agency, etc.

The NAS/NRC COMRAA (Committee on the Organization and Management of Research in Astronomy and Astrophysics) study made an explicit recommendation for an AAAC-like committee, although that committee strongly disagreed with the one-agency approach. The AAAC was established by Congress in the 2002 NSF Authorization Act and formally initiated late last year. The Committee membership is 13, and the members are selected by OSTP, NSF, and the National Aeronautics and Space Administration (NASA). DOE is currently involved informally, but a formal role is expected in the future. The Committee must submit a report by March 15 of each year to Congress and the agency directors/administrators; its first report was issued in March 2004.

Congress specifically called upon the Committee to

"(1) assess, and make recommendations regarding, the coordination of astronomy and astrophysics programs of the Foundation and the National Aeronautics and Space Administration;

"(2) assess, and make recommendations regarding, the status of the activities of the Foundation and the National Aeronautics and Space Administration as they relate to the recommendations contained in the National Research Council's 2001 report ... and the recommendations contained in subsequent National Research Council reports of a similar nature; and

"(3) not later than March 15 of each year, transmit a report to the Director [of NSF], the Administrator of the National Aeronautics and Space Administration, and the Committee on Science of the House of Representatives [inter alia]."

That same language defines the membership to "consist of 13 members, none of whom shall be a Federal employee, including (1) 5 members selected by the Director; (2) 5 members selected by the Administrator of the National Aeronautics and Space Administration; and (3) 3 members selected by the Director of the Office of Science and Technology Policy."

It also calls upon the Committee to "coordinate with the advisory bodies of other federal agencies, such as the Department of Energy, which may engage in related research activities" and to "convene, in person or by electronic means, at least four times a year."

AAAC is focused on implementation of the Decadal Survey(s) and other comparable NAS/NRC reports. This year, its recommendations covered two areas:

- Broad recommendations for implementation of science programs and
- Specific recommendations for programs in the Decadal Surveys.

NASA was particularly concerned about its funding profile in the FY05 budget and about the science impact of the Hubble Space Telescope (HST) decision.

The NSF focused on high-priority programs in the Decadal Survey and on the complementary and synergistic role of ground plus space (e.g., as shown by the HST plus an 8- to 10-m-class ground-based telescope to investigate supernovae and dark energy):

- Giant Segmented Mirror Telescope (GSMT, a 30-m telescope), which is moving forward on technology development to ensure GSMT operational on a James Webb Space Telescope (JWST) timescale (the GSMT and JWST are the highest-rated programs in the Decadal Survey);
- Large Synoptic Survey Telescope/Joint Dark Energy Mission (LSST/JDEM, dark-energy projects), which is to provide support for implementation of both programs, enabling a timely and coordinated effort on dark energy; and
- Advanced Technology Solar Telescope/Solar Dynamics Observatory (ATST/SDO, solar telescopes), providing scientific synergy.

Enabling technologies and capabilities are key in achieving all this.

Current activities of the Committee include:

- Following up the 2004 report, getting feedback from OMB, OSTP, Congress, agencies, etc.
- Endorsing an effort involving GSMT and JWST science working groups to identify complementary/synergistic science goals and capabilities.
- Setting up a cosmic microwave background radiation (CMBR) task force as a joint subcommittee of AAAC and HEPAP to outline a path for CMB polarization studies and programs.
- Recommending the establishment of a dark-energy task force (DETF), iterating on charge, members, timescale, etc. This would be a third step after the strong science case presented in the *Quarks and the Cosmos* and the broad implementation plan. This effort needs a roadmap.

- Expressing interest/concern about funding for major projects at the NSF, specifically the time scale for the implementation of the Decadal Survey recommendations.
- Expressing concern about the impact of NASA's changes on space science and their impact on current strategic plans for roadmaps based on Decadal Surveys and *Quarks and the Cosmos*. About 25% of the NASA budget goes to earth science.

The focus of the AAAC is on implementing the Decadal Surveys and NRC studies. Therefore, interaction between the AAAC and the Committee on Astronomy and Astrophysics (CAA), which is responsible for oversight of the Decadal Surveys, is crucial. The two have common goals: the implementation of the Decadal Surveys and community priorities as reflected through NRC/NAS reports/studies. How can CAA and AAAC best work together? CAA works within a broad framework; AAAC deals with implementation. AAAC is designed to help agencies implement strategic goals through tactical efforts. In addition, AAAC must interact with other agencies' FACA (Federal Advisory Committee Act) committees (HEPAP, the Space Science Advisory Committee and its subcommittees; Mathematical and Physical Sciences Advisory Committee, etc.). Such interaction is needed to enhance the likelihood of programs moving ahead by developing common priorities, approaches, and language. Note that AAAC will evolve to formally include DOE for astronomy and astrophysics programs; in the meantime, it wants to establish and continue a dialogue.

Gilman said that HEPAP wanted to be involved in the CMB task force but found out that the membership had already been set. Better communication is needed in the future, especially on dark energy. Illingsworth noted that the charge needs to be looked at; membership should be scrutinized. It takes time but is worth it. Dehmer concurred that this was handled poorly and said that it will be done better in the future. He noted that Illingworth's presentation at HEPAP was very positive. Illingworth said that the CMB section was driven by an immediate need. Dehmer added that FACA governs how advice is given to the Government.

Gilman asked what should be written in the letter about the Linear Collider. He suggested:

1. Give congratulations to Barish.
2. Recognize acceptance by the community of the recommendation of the cold technology.
3. Look forward to a Global Design Effort.
4. Express pleasure in seeing NSF and DOE working together on university research.
5. Look to the FALC to move with parallel authority the science and technology of the international design effort.

Kim commented that a lot of excitement was engendered by the technology choice. The letter should say something about the need to maintain the enthusiasm.

Dragt said that it should mention what is expected to be accomplished at the next HEPAP meeting.

Kim asked what level of R&D would be necessary. Tigner responded that that would not be known before the next HEPAP meeting; it would take some time to arrive at an agreement.

Dragt asked about the reactor experiment. Kayser replied that it could be done anywhere. One builds two detectors and looks to see if the neutrino disappears by the time it reaches the far (1-km-distant) detector. It could be done in the United States, Japan, China, South America, or Europe. Many sites are under discussion.

Gilman asked what action should be taken. HEPAP need not endorse the report; it is not a report to HEPAP. The Committee should wait for the final report to see what it says. Kayser replied that the output of the study will be input to the policymakers, but further steps will require input from committees like this one.

Hewett inquired if the report would have supplementary data. Kayser replied that it will have a timeline but not cost estimates.

Meyers noted that scientific goals may be presented and proposals may start filtering in and asked if a coherent process for shepherding such proposals was envisioned. Hewett replied that P5 might be a good place for that task. Gilman stated that this subject is too preliminary for P5. Butler commented that a roadmap needs to be developed that could be ready for such proposals to avoid chaos. Staffin noted that HEP is not the only player in neutrinos. Neutron Physics (NP) is another interested party. He questioned whether a science advisory group should report to both HEPAP and NSAC (the Neutron Physics Advisory Committee). Gilman said that HEPAP should do more than just thank APS for the study; it needs to fill out the roadmap for neutrinos. Witherell stated that the agency has to

Friday Morning

The meeting was called to order at 8:32 a.m. Gilman asked **Rene Ong** to present the report of the Scientific Assessment Group for Experiments in Non-Accelerator Physics (SAGENAP), which is now a HEPAP subcommittee.

Exciting research is occurring at the physics/astronomy interface. SAGENAP arbitrarily grouped projects into four broad areas:

1. Dark energy and cosmic microwave background,
2. Dark matter,
3. Very-high-energy (VHE) particle astrophysics, and
4. Neutrinos.

Recent highlights of the science include the discovery of an acceleration in the expansion of the universe ("dark energy"), a pinning down of the key cosmological parameters, the discovery of two manifestations of neutrino oscillations, the detection of cosmic γ-ray sources and 10^{20}-eV particles coming from the center of our galaxy, and increasingly improved constraints on particle dark matter.

A charge letter from Ray Orbach and Michael Turner requested "that the High Energy Physics Advisory Panel (HEPAP) establish a subpanel to assess projects in experimental non-accelerator physics … SAGENAP's role is to provide one view of which projects are worthy of further, in depth, consideration for funding by the agencies. SAGENAP's primary consideration is the scientific merit of the project.

"SAGENAP will assess projects in three categories:

1. Projects in the conceptual phase;
2. Projects that are ready to request agency funding for concept studies, design and development, or construction;
3. Ongoing projects funded by the above-named agencies.

"For projects in category 3, SAGENAP will:

- Assess progress and any scientific issues on the ongoing project and identify any areas of concern for agency attention.

"For projects in categories 1 and 2, SAGENAP will:

- Assess the scientific merits of the project.
- Assess the readiness of the project to request funding for concept studies, design studies, or construction.
- Assess the scientific and technical goals of the project in the context of related activities in the field.
- Assess the scientific, technical, organizational, and management capabilities of the project team."

The last two tasks are difficult because only brief information is available, there are many projects, and there are only 10 SAGENAP members. A broad subject coverage is represented by the membership, but there are only one or two members in each discipline.

The group heard reports on 26 projects. In categories 1 and 2, they included the Dark Energy Survey, Destiny, LSST, Polarbear, Quiet, ASHRA, Auger Project N, HAWC, Telescope Array/TALE (high-energy gamma rays), ICARUS, and LANNDD. In Category 3, they included SNAP R&D, CDMS II, DRIFT R&D, XENON R&D, ZEPLIN II, Auger Project S, HiRes, Milagro, STACEE, VERITAS, EXO R&D, KamLAND, and Super-Kamiokande. It also heard general reports on the accelerator, reactor-based neutrino effort; double-beta decay, and electron electric dipole moment (EDM) projects.

SAGENAP adopted the strategy that scientific assessment is most important, and a scientific and programmatic context is needed. Individual project assessments were carried out at three levels:

- A "heads-up," giving an assessment of the science, asking if they can do what they propose, and providing feedback on concerns for a future proposal.
- A "proposal" (the only one being the LSST), giving an assessment of the science, asking if they can do what they propose, and providing a detailed review of the project (on which SAGENAP spent a lot of time).
- A status report, reviewing a summary of the progress.

A decision was made not to prioritize; the group did not have enough time and information to do a good job. Individual perspectives rather than consensus were sought. The resulting report is dense and project-oriented; the recommendations are not detailed or highlighted.

These assessments were carried out at a three-day meeting in April. Materials were received from each project. A website with public and private sections was established at http://astro.ucla.edu/~sagenap/. Four teleconferences and copious e-mails were also employed.

In dark energy, the group heard one proposal from LSST, two heads-ups about the Dark Energy Survey and Destiny, and one status report on SNAP R&D. Dark Energy is a mystery of great scientific importance. A variety of different techniques and approaches are being employed:

- Supernova Ia, weak lensing, galaxy clusters, etc.;
- A variety of space-borne and ground-based techniques; and
- Multiple wavebands, agencies, and communities involved.

SAGENAP recommends a Roadmap Study to provide context for this important area.

SNAP (SuperNova Acceleration Probe) is a relatively mature and well-developed concept.

LSST (Large-aperture Synoptic Survey Telescope) is an 8-m wide-field ground-based telescope. It probes cosmology with weak lensing, supernovae etc. It has a great capability to study transients. It has a larger field of view than all the other concepts, an enormous data rate, and a large data volume. SAGENAP concluded that LSST will explore outstanding science. It is an excellent concept and has a very strong team. Its R&D funding is well-motivated. There are some concerns in science management, simulations, camera R&D and testing, and data handling. The relevant agencies should define a coherent process to move LSST forward through the R&D (and possibly D&D) phase.

The Dark Energy Survey (DES) is conducting dark-energy measurements with lensing, supernovae, and clusters. The point is to build a new 3-deg^2 camera and take it to an existing telescope. It is a partnership among the South Pole Telescope, universities, and FNAL. Its science goals are excellent; the team is strong; and the plan is good. There is some concern on costs and software. A roadmap is needed to put this project into the proper context.

Destiny is a space-borne instrument designed to study supernovae Ia. It is simpler and currently less mature than SNAP. It is relatively simple (grism), possibly has a lower cost, but may have larger systematic errors. Simulations and trade-off studies are required; the team needs strengthening.

Polarbear and QUIET both attempt to measure CMB polarization (B-modes). They are at several stages. Polarbear is a 3-m telescope with a monolithic detector design. The technique is novel and promising. QUIET would move an existing 7-m telescope to Chile and uses compact MIMIC technology. Both projects address important science, are being tackled by a number of groups, and merit funding in the broad picture.

In dark matter, status reports were heard from CDMS II, DRIFT R&D, XENON R&D, and ZEPLIN II. Many projects worldwide are searching for particle dark matter. They should be supported. Xenon may be preferred technology for very large (ton) experiments. SAGENAP recognizes that good progress has been made by the XENON and ZEPLIN groups, so it encourages support for R&D towards a large liquid xenon detector.

In VHE particle astrophysics, SAGENAP heard four heads-ups from ASHRA, Auger N, HAWC, and Tel Array/TALE and five status reports from Auger S, HiRes, Milagro, STACEE, and VERITAS. HiRes is probing the mystery of ultrahigh-energy cosmic rays; currently there is a discrepancy between experiments on the flux of events $>10^{20}$ eV. But the upcoming projects Auger S and VERITAS look very promising. Three of the heads-ups involved the study of particles at $>10^{20}$-eV scale in the Northern Hemisphere. SAGENAP is unable to say that any of these projects should move forward, but there is a possible need for a study to put these efforts into a general context.

The High-Altitude Water Cherenkov (HAWC) is a wide-field γ-ray telescope and instrumented pond at high altitude. It uses the water Cherenkov technique (Milagro). A science case is not yet very strong; the team needs to justify what new physics HAWC would provide beyond the existing and planned suite of γ-ray telescopes.

The All-sky Survey High Resolution Air-shower (ASHRA) detector is a cosmic-ray experiment for studying VHE γ-rays and ultrahigh-energy (UHE) cosmic rays. The concept has not yet demonstrated significant scientific advance. Detailed simulations are needed. Japan has provided funding; the U.S. role in the project needs to be understood.

The Auger Project North is a UHE cosmic-ray detector, a second observatory for complete-sky coverage. It uses a ground array and N_2 fluorescence. The science case has not yet been established. There is a need to demonstrate why Auger South is not sufficient to answer the key scientific questions. There are also some concerns regarding design changes and the site.

Telescope Array/TALE (telescope array low energy) uses a N_2 fluorescence detector to study UHE cosmic rays. Part of the Telescope Array is in Utah; a low-energy extension is proposed. The science and technical cases for TALE is not yet strong. The overall context of Telescope Array is not clear. It has significant funding from Japan, and the role of U.S. institutions must be understood.

In neutrinos, SAGENAP heard heads-ups from ICARUS (Imaging Cosmic And Rare Underground Signals) and LANNDD (Liquid Argon Neutrino and Nucleon Decay Detector); status reports from EXO R&D, KamLAND, and Super-K; and presentations on double-β and reactor/accelerator neutrino experiments. The current experiments are outstanding. The future scientific promise is great, and these efforts should be supported. The double-β decay search is very important and should be aggressively pursued. SAGENAP agrees that the motivation for measuring

θ_{13} is compelling. A reactor experiment appears to be a good bet, but that was not part of the group's charge. A reactor experiment needs to be considered in the overall context, and the APS study is close at hand.

ICARUS is a planned 3-kt liquid-argon time projection chamber (600 tons of argon exist) to investigate heavy-neutrino decay and neutrino physics (including a neutrino beam from CERN). SAGENAP supports the physics goals. The U.S. contribution should be continued in coordination with the actual detector schedule.

For the longer term, LANNDD is a concept for a liquid-argon detector in the 100-kt range to study heavy-neutrino decay, long-baseline neutrinos, and supernova neutrinos. Liquid-argon detectors could play an important role in future neutrino experiments, an area in which the United States should regain technical strength. SAGENAP supports the idea of an increased R&D effort, subject to safety considerations and requiring a stronger group.

The central findings of the study are

- Roadmapping and a broader context are needed, especially in dark energy.
- In the Dark Energy and CMB projects, spectacular results have led the group to recommend the pursuit of well-motivated new projects.
- The LSST is very strong, leading to the conclusion that it is reasonable to proceed with R&D.
- Current dark-matter projects are going well, and the limits are rapidly improving. The group encourages the development of a large xenon detector.
- VHE-particle astrophysics projects are exhibiting lots of activity and producing exciting recent results. A general context is needed to understand the 10^{20}-eV mystery.
- Neutrino projects are producing great results and have great potential. A reactor experiment seems very sensible, and liquid-argon technology merits an increased R&D effort.
- Two electron electric dipole moment projects were identified. They reflect good science, but are best done in the atomic physics community.

Roadmapping is needed; the science, measurements, and experimental techniques need to be mapped onto projects. SAGENAP needs yearly meetings with a broader context; biennial meetings are not adequate. It also needs a larger panel or multiple panels; subgroups focused on specific areas are possible but would limit understanding of the broad context. There is a need for coordination with and inclusion of other areas, such as nuclear physics and astronomy.

In summary, this is a very exciting time. The scientific potential is outstanding, and there is a remarkable range of experimental opportunities. It is important to think about how to plan for the future. SAGENAP's report is a draft, and suggestions are welcome. The community is waiting for feedback.

Langacker said that SAGENAP had done a wonderful job and congratulated them. The electron EDM projects are extremely important, but condensed-matter reviewers do not understand them. HEP should expedite research in that field.

Baker asked if there were groups that did not get to air their concerns and projects. Ong responded that SAGENAP was not formed to do a roadmap and broad-look study. It saw projects that were already on the table. A more careful study could be done. SAGENAP got more than 60% of the projects; 150 people attended.

Mallik asked if there was a bias that excluded certain projects. Ong replied that some projects were not presented to SAGENAP this year. For example, IceCube and GLAST had been presented to SAGENAP in previous years. This timing was not appropriate for them. Ritz pointed out that the agencies themselves decided what projects would be represented.

Kim asked if there could be a study on dark matter/energy for the whole community. Ong said that the main focus now should be on the variety of techniques and numbers of different experiments, as opposed to the science. What is needed now is to determine how best to get to the answers.

Montgomery asked if SAGENAP was thinking about a 5-kt liquid-argon detector or something larger. Ong said that SAGENAP did consider the importance of the liquid-argon technique for much larger volumes, but that right now the United States is not well invested in this area and not prepared to move towards a much larger detector.

Staffin jokingly asked if HEP was getting involved with a reputable bunch of people. Ong answered that these are astronomers who are strong and have key roles in the field of cosmology.

Ritz noted that it would be useful to get feedback from those who went through this process. The community needs to understand who is in charge at the agencies. He would caution against subcommittee "eruptions." SAGENAP should meet more often rather than set up more groups. It would also be helpful to see how SAGENAP relates to the P5 Committee. Ong noted that there is little feedback to the particle-physics community. That situation should be corrected.

Dehmer observed that astrophysics is going through a period of rapid growth, which is occurring in a number of agencies. SAGENAP is doing an important job to guide those agencies. This is an arena in which HEPAP can really advise. HEPAP needs to make SAGENAP more robust during the next few months.

Meyers commented that he had always liked the ad hoc nature of SAGENAP. He was concerned that it is getting bigger than the group it reports back to. It is connected scientifically, but some of its purview is getting to be outside the expertise of HEPAP. Ong suggested that, if need be, the CMB could be removed from SAGENAP's purview.

Gilman asked for an assessment of the report. There being no comments, he asked for those wishing to accept the report. The motion to do so was passed unanimously.

Gilman introduced **Raymond (Chip) Brock** to present an update on the activities of the Human Resource Study Group, which was formed the previous spring. The Group's charge asked it to poll university and laboratory groups about their level-of-effort expectations for the next 5 years. Letters were sent to each group; 236 mailings were sent out. Replies are due by the end of September. A FAQ website was established.

Principal investigators (PIs) were asked specifically about the need for physicists. The letters set ground rules for counting physics positions. Responses have already been received from 22 of 122 DOE people and 15 of the 118 NSF people; 33 experiments were polled. Similar instructions were given to the spokespeople for the different types of institutions. The Study Group is now discussing how to process the data coming back. It expects to do a lot of quality control on the data. It needs to establish milestones. It hopes to have results by the next HEPAP meeting.

Hewett asked how they captured the smaller experiments. Brock responded that they just tried to make sure they got all the big experiments.

Kim noted that a workshop for the LHC was held that worked out very well to scope out the human resources that needed follow-up.

Gilman asked **Elizabeth Simmons** to report on the Aspen workshop.

The Aspen Workshop on Physics Education and Outreach was a two-week session for K-12 teachers during the summer of 2004. It was funded by the NSF Directorate for Mathematical and Physical Sciences, the Institute for Complex Adaptive Matter, the APS Topical Group on Statistical and Nonlinear Physics, and the APS Division of Particles and Fields.

One topic the workshop explored was the benefit of popularizing particle physics. By popularizing particle physics is meant conveying the inherent excitement and fundamental goals of particle physics to the public and helping the public appreciate the beauty and creativity of the scientific endeavor in order to inspire the next generation of scientists, promote scientifically informed public policy, maintain support for continued funding of physics, and help our families understand why we love physics. The public that must be addressed is very broad.

All physicists should get involved; the stakes are high. Consider the NSF's Merit Criteria in assessing science: (1) intellectual merit and (2) the broader impact of advancing discovery and understanding while promoting teaching, training, and learning; broadening the participation of underrepresented groups; enhancing the infrastructure for research and education; disseminating results broadly; and conferring benefits upon society. The NSF gives some examples of interaction between research and education:

- Help in training of K-12 science and math teachers and
- Encourage student participation at conferences

and in the broad dissemination of research results:

- Make data available electronically and
- Participate in multi- and interdisciplinary conferences.

Education and outreach activities can satisfy the "broader impact" criterion for an NSF proposal. Physicists already do some such activities as a matter of course, and many others will directly enhance our research efforts.

The workshop also considered how to choose what to do. The big thing to come out of the workshop was a recognition of the need to match up the volunteer's interests and talents with the intended audience. Ideas need to be framed accordingly: relate the volunteer's favorite topic to the audience's interests, tailor communications to the audience's level, and choose actions that help meet the audience's goals.

One should contact the potential audience and find out what education or outreach activities and materials its members are already using and what they need in addition. Is it extension/enlargement of existing programs, new programs or materials, coverage of different topics, translation of existing materials into another language, or help making practical use of new information? Then find the resources you need to assist you: existing items and local individuals, organizations, or informal networks that you can partner with. Look for an existing outreach effort; local museums, radio stations, scout troops, or rotary clubs looking for volunteers with science expertise; other HEP folks in your area interested in outreach; and a local physics students' club.

Physicists must be aware of several common pitfalls. The expert's knowledge and assumptions and the lack of solid metaphors and analogies can be barriers to effective communication. The format of presentation should avoid "reading hypnotism"; jokes; jargon; and surprises, action, and suspense.

Follow-up is needed to determine whether the data provided arrived in an understandable format, whether the recipient can cope when donated equipment goes haywire, and whether the audience knew where to get more information.

The workshop reviewed what others had done successfully. An important technique is to involve the audience in a lecture with demonstrations, breakout groups, and turning *them* into an experiment.

Involve students and teachers in ongoing HEP research. One such effort is the Quarknet Collaboration, which brings students and teachers into the laboratory and provides lesson plans and online activities. It is now in its sixth year, has participants in 25 states and Puerto Rico, has 52 centers with 208 mentors and 507 teachers; it is associated with 11 experiments conducted at 7 DOE laboratories and CERN. (See quarknet.fnal.gov.)

Create research consortia based in high schools. An example of such a consortium is the cosmic-ray experiment that holds a summer workshop to build equipment for a cosmic-ray telescope that is put on the roof of the school to detect cosmic-ray showers. This effort has local control but national impact.

One can also convey scientific content to those skilled at reaching the public by educating people at museums and planetaria about recent research results and by creating Web resources.

She concluded by encouraging physicists to think broadly about education and outreach. Most physicists are probably already doing some education and outreach, but more is always needed. There are many ways to contribute. One can join existing programs or create new ones. It should be borne in mind that work will have impact if it is prepared well. The effort involved will be minimized by taking advantage of existing resources. One should support the efforts of others by encouraging students and postdocs to become involved, helping out with a program a junior colleague is starting, and making sure physics departments value education and outreach when promotions and raises are discussed.

Follow-up to the workshop includes

- Writing articles and talks on lessons from the workshop,
- Compiling a searchable database of HEP public lectures,
- Initiating a Listserve/archive on education and outreach,
- Providing more training at APS/AAPT (American Association of Physics Teachers) New Faculty Workshop and Chairs Workshop,
- Conducting outreach sessions at all Division of Particle Physics meetings, and
- Producing "Einstein for a Day" national outreach days in March 2005 for the World Year of Physics.

Staffin said that this type contact with the broader public is essential.

Dragt asked if there was any attempt to target the older voter. Simmons replied, no, but we need to do that.

Ruchti noted that the community needs to take advantage of retired physicists. It also needs to come to terms with what "broader impacts" means. Workshops strengthen the effort by bringing together people from diverse backgrounds and with diverse capabilities. It is humbling to realize that what you are interested in is not interesting to others.

Ritz commented that physicists and other speakers do not even recognize the pitfalls as they walk into them. NASA uses feedback to determine what works and what does not. It is difficult to determine how to measure the effectiveness of efforts like Web pages. Simmons said that she should have mentioned that need. Interactions.org forms a collective well that journalists can go to in order to get information about science.

Strauss pointed out that Lions and Rotary are very effective groups to speak to because they go and talk to others, like congressmen.

Gilman asked how Einstein for a Day will work. Simmons replied that any university or laboratory that has an outreach program is being urged to do it simultaneously during this week.

Ferbel pointed out that there is no statement that DOE clearly supports educational efforts. HEPAP should pressure DOE to recognize how essential early education is.

A break was declared at 10:16 a.m. The meeting was called back into session at 10:35 a.m. The tentative time for the next meeting was set as February 14-15, 2005, a Monday and Tuesday. Gilman introduced **Mary Anne Scott** to speak about the Energy Science Network (ESnet).

ESnet has been around since the mid-80s; it formerly was FESnet and HEnet. Its mission is to provide interoperable, effective, and reliable communications infrastructure and leading-edge network services that support missions of the Department of Energy, especially the Office of Science (SC). Essentially all of the national data traffic supporting U.S. science is carried by two networks: ESnet (which serves government laboratories) and Internet-2/Abilene (which plays a similar role for the university community).

Between 10,000 and 100,000 researchers in the United States use ESnet, mainly SC programs. However, ESnet also carries traffic for the National Nuclear Security Administration (NNSA), all the U.S. national laboratories, hundreds of universities, and hundreds of foreign institutions.

Studies in January 2003 and February 2004 showed that DOE is a net supplier of data because DOE facilities are used by universities and commercial entities:

Type of Host ESnet Connected to	Outgoing Traffic (%)		Incoming Traffic (%)	
	2003	2004	2003	2004
Commercial	21	14	14	12
R&E (mostly universities)	17	10	10	13
International	9	26	4	6

Daily usage data show that a small number of science users (predominantly SLAC and Fermilab) account for a significant fraction of all ESnet traffic. Such traffic is not transient: Daily and weekly averages are about the same.

ESnet is important because it enables thousands of DOE, university, and industry scientists and collaborators worldwide to make effective use of unique DOE research facilities and computing resources independent of time and geographic location. ESnet provides direct connections to all major DOE sites and access to the global Internet. It has experienced a growth in user demand of a factor of more than 10,000 since the mid-1990s (a 100% increase every year since 1990). It is architected to move huge amounts of data among a small number of sites and to provide high-bandwidth-peering access to U.S., European, Asia-Pacific, and other research and education networks. This capability is not commercially available. ESnet's objective is to support scientific research by providing seamless and ubiquitous access to the facilities, data, and colleagues.

ESnet is a visible and critical piece of DOE science infrastructure. If ESnet fails, tens of thousands of DOE and university users know it within minutes if not seconds. High reliability and high operational security are required in the network operations and ESnet infrastructure support.

ESnet is a large data-communication ring that passes through hubs in the Bay Area in California, Chicago, New York, Atlanta (a peering point for Abilene), and El Paso before returning to the Bay Area. Many other links, loops, and networks are connected to it along the way as well as four high-speed peering points for transmissions to other large-scale networks in the United States, Europe, and Asia. ESnet is a Tier 1 router connected to virtually every other peering point on the Internet.

ESnet is managed by the Energy Science Network Steering Committee (ESSC), which has representatives from all SC offices plus other stakeholders [HEP is represented by Larry Price of Argonne National Laboratory (ANL) and Richard Mount of SLAC]. ESSC guides ESnet by telling it what the users need. The ESnet Site Coordinators Committee ensures the right operational "sociology" for success. This is all complex and specialized in both the network engineering and the network management.

ESnet provides wide-area-network (WAN) security and cyberattack defense by installing filters in routers in the network.

Planning workshops are held to determine where the network is going in the future. The vision that has emerged from the recent workshops was of a seamless, high-performance network infrastructure in which science applications and advanced facilities interconnected to terascale computing, petascale storage, and high-end visualization capabilities. This advanced network facilitates collaborations among researchers and interactions between researchers and experimental and computational resources. With such connectivity, science, especially large-scale science, moves to a new regime that eliminates isolation, discourages redundancy, and promotes rapid scientific progress through the interplay of theory, simulation, and experiment.

The first workshop was on network and middleware needs of DOE science. Surveys of the user community have shown that the quantitative science requirements for networks are all converging to the same input of 1000 GBs. Given that, a roadmap was developed. The number-one driver for continuing advancements in networking and middleware was petabyte-scale experimental, and simulation data systems will be increasing to exabyte-scale data systems. The vision that emerged was of a scalable, secure, integrated network environment for ultra-scale distributed science to make it possible to combine resources and expertise to address complex questions that no single institution could manage alone. A network strategy is needed as well as a revisiting of the governance model to ensure SC-wide coordination and advisory-committee involvement.

In the future, ESnet will be upgraded to accommodate the anticipated increase from the current 100% per year traffic growth to 300% per year over the next 5 to 10 years. This upgrade is priority number 7 out of 20 in DOE's *Facilities for the Future of Science: A Twenty-Year Outlook*. Based on the requirements, ESnet must address four items:

1. Capable, scalable, and reliable production IP networking to provide university and international collaborator connectivity and scalable, reliable, and high-bandwidth site connectivity;
2. Network support of high-impact science, including provisioned circuits with guaranteed quality of service (e.g., dedicated bandwidth);
3. Evolution to optical switched networks in partnership with the UltraScienceNet and in close collaboration with the network R&D community; and
4. Science services to support Grids, collaboratories, etc.

Reliability is becoming increasingly important. To provide that reliability, the desired future architecture has redundant (two concentric and interconnected) rings.

The Particle Physics Data Grid (PPDG) project has achieved robust, sustained, hands-off, production data transfer of terabytes of data using GridFTP (file transfer protocol) and SRM (scalable reliable multicast) implementations. It has also achieved Grid-based job scheduling and execution based on Condor-G, DAGMan, and GRAM.

In conclusion, ESnet is an infrastructure that is critical to DOE's science mission. It is focused on the SC laboratories but serves many other parts of DOE. ESnet is working hard to meet the current and future networking need of DOE mission science in several ways: evolving a new high speed, high reliability, leveraged architecture and championing several new initiatives that will keep ESnet's contributions relevant to the needs of the SC community.

It is important that ESnet has a good relationship with the other portions of DOE that it serves. HEPAP and the other advisory committees need to be involved in the early identification of network requirements that take into consideration evolving programs and new facilities. HEPAP needs to interact with the HEP representatives on the ESSC; they are HEP's voice in determining where ESnet is going. HEPAP should also participate in ESnet's management activities; the method of ESnet governance will be changing. The next ESSC meeting will be early in 2005 in the Nation's Capitol Area.

Gilman asked when the second backbone ring will be up and running. Scott said that it would probably be 2 to 3 years from now. Gilman asked if there was a need for additional links among ESnet, HEPAP, and HEP. Scott replied that a higher level of governance that might have representatives from advisory committees was being discussed. A peer review of the program is needed; the next review should include advisory-committee representatives. Price noted that one suggestion is that one member of ESSC comes from or is appointed by the advisory committees.

Butler commented that most of the network utilization is now from outside the laboratories and asked how ESnet was going to plan for this. Scott answered that it will always be a question of specific sites. ESnet's management has a good appreciation of end-to-end performance. They are instituting measurement capabilities throughout the network to assess performance. Butler asked if ESnet knew what the university community is doing. Scott replied that ESnet and the university community were working on the problem together and that there is a reasonable accord among these communities.

Byon-Wagner asked what ESnet was doing on network research. Scott replied that ESnet does not have a large program, but it is developing research capabilities. Some results are already out on the Science Grid.

Peggs asked if capacity drove traffic or the other way around. Scott responded, both. There are people that are not doing projects because the capabilities are not there yet.

Dragt asked how DOE researchers will communicate with CERN when the LHC comes online. Scott said that that problem was under discussion. It is not known what the infrastructure will look like or will come from but ESnet was considering several alternatives. Price stated that the work of Harvey Newman at Cal Tech will handle some of the traffic with CERN. Scott noted that, in the long run, one must recognize the need for and utility of this infrastructure.

Gilman thanked her and turned the subject to the Committee's letter to Turner and Staffin. He asked rhetorically what topics should be included:

- DOE and NSF coordination on the ILC
- Writing Group should have an international component
- Recognize the globalization of resources and endorse the appropriate exchange of technical skills
- Ask for a clear understanding of the schedule of the need for human resources
- They will also need planning resources

- Ask how we can help specifically

Kim asked how flexible funding was in the face of uncertainties. Staffin replied that, up to a certain amount, one can ask for increases. Also, one can request formal reprogramming.

Ritz asked about the NRC/NAS study. Gilman replied that it is not in the purview of HEPAP. HEPAP should encourage colleagues to support the study with information, but it cannot do anything officially. Staffin said that he anticipated that this will be a very useful study.

Gilman noted that SAGENAP had noted this effort has grown rapidly. There are questions of how to manage it. There is a clear need for more oversight and awareness of developments. Ruchti commented that there are some missing parts to the input to SAGENAP and that more background information is needed. Gilman suggested that a roadmap and an expanded membership would address that problem. Meyers said that he was overwhelmed by what that group is being asked to do; at some point it might need more focused subcommittees. Gilman replied that HEPAP should ask the agencies how they would like to see this area develop and what HEPAP can do to help.

The meeting was adjourned at 11:46 a.m.

The minutes of the High Energy Physics Advisory Panel meeting held at the Hilton Washington Embassy Row, Washington, D.C. on September 23-24, 2004 are certified to be an accurate representation of what occurred.

Frederick J. Gilman

Frederick Gilman
Chair
High Energy Physics Advisory Panel